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VIII. *Experiments and Observations on the Motion of the Sap in Trees. In a Letter from Thomas Andrew Knight, Esq. to the Right Hon. Sir Joseph Banks, Bart. K. B. P. R. S.*

Read February 16, 1804.

MY DEAR SIR,

IN the Observations on the Descent of the Sap in Trees, which I last year took the liberty to request you to lay before the Royal Society, I offered a conjecture, that the vessels of the bark, which pass from the leaves to the extremities of the roots, were, in their organization, better calculated to carry the fluids they contain towards the roots than in the opposite direction. I had not, however, at that time, any experiment directly to support this supposition; but I thought the forms generally assumed by trees in their growth, evinced the compound and contending actions of gravitation, and of an intrinsic power in the vessels of the bark, to give motion to the fluid passing through them. In the account of the experiments which I have now the honour to address to you, I trust I shall be able to adduce some interesting facts, in support of that inference.

Having selected, in the spring of 1802, four strong shoots of the vine, growing along the horizontal trellis of my vinery, I depressed a part of each shoot, whilst it was soft and succulent, about three inches deep, into the mould of a pot placed beneath it for that purpose; but without making any wound, or incision, in the young shoots thus employed as layers.

In this position they remained during the succeeding summer ; and, in the autumn, had nearly filled the pots, which were ten inches in diameter, with their roots. As soon as the leaves had fallen, the layers were disengaged from the parent stocks ; and about five inches of wood, containing one bud, were left, both at the proper and the inverted end of each layer. Every bud was also, by previous management, made to stand at an equal distance from the mould in the pots, and with an equal elevation, of about thirty-six degrees. About one inch of wood was likewise left at each end of every layer, beyond the buds.

In the succeeding spring, the buds vegetated strongly, both at the proper and at the inverted ends of the layers, as the experiments of HALES and DU HAMEL had given me reason to expect ; and, in one instance, the bud at the inverted end of the layer grew with greater vigour than that at its proper end : but the growth of these buds was not the object which I had in view.

I have already stated, that nearly an inch of wood was left at each end of every layer, beyond the bud ; and, to this wood, at the inverted ends of the layers, my attention was chiefly directed : for, if the vessels of the bark possessed the powers I attributed to them, I concluded that the sap would be impelled to the inverted ends of the layers, and be there employed in the production of new wood, and roots ; and, in this, my expectations were not disappointed. At the proper end of the layers, the wood immediately beyond the buds became dry and lifeless, early in the succeeding summer ; the stems also, between the buds and the mould in the pots, increased in size as usual ; and nothing peculiar occurred. But, at the inverted end, appearances were extremely different : new wood here accumulated rapidly beyond

the buds, and numerous roots, of considerable length, were emitted, whilst no sensible growth took place between the base of the young shoots and the mould in the pots.

It having been proved by DU HAMEL, that inverted parts of trees readily emit roots, I expected to derive further information from cuttings of this kind: I therefore planted, in the autumn of 1802, forty cuttings of the gooseberry-tree, and an equal number of the common currant-tree; one half of each being inverted. Of the former, not one of the inverted cuttings succeeded; whereas few of the latter failed; and in these I had an opportunity of observing the same accumulation of wood above the bases of the annual shoots, and the same mode of growth, in every respect, as in the inverted vines; except that no roots were emitted at their upper ends. The same thing occurred, without any variation, in inverted grafts of the apple-tree.

If it be admitted, according to the theory I have on a former occasion laid before you, that the sap descends from the leaves through the vessels of the bark, and that such vessels are, in their organization, better calculated to carry their contents towards the original roots than in the opposite direction, it will be extremely easy to explain the cause of the accumulation of wood, and the emission of roots, above, instead of below, the base of the annual shoots. The vessels of the bark (the *vaisseaux propres* of DU HAMEL) commencing in the leaves, were formerly traced by M. MARIOTTE, and subsequently by myself, (being ignorant of his discovery,) to the extremities of the roots; and, when a cutting, or tree, is planted in its natural position, the sap passes downwards through these, to afford matter for new roots, and to increase the bulk of those already formed, having given proper nutriment to the branches and trunk in its descent.

But, in the inverted cutting, or tree, these vessels become inverted; and, if their organization be such as I have supposed it, a considerable part of that fluid, which naturally descends, will be carried upwards, and occasion the production of new wood, above, instead of below, the junction of the annual shoot with the older wood, as in the experiments I have described. The force of gravitation will, however, still be felt; and, by its agency, sufficient matter to form new roots may be conveyed to those parts of the inverted cutting, or tree, which are beneath the soil. Besides, if we suppose a variation to exist in the powers or organization of the vessels which carry the sap towards the root, we may also attribute, in a great measure, to this cause, the different forms which different species or varieties of trees assume; for, if the fluid in these vessels be impelled with much force towards the roots, little matter will probably be deposited in the branches, which, in consequence, will be slender and feeble, as in the vine; and there is not any tree that has been the subject of my experiments, in which new wood accumulated so rapidly at the upper end of inverted plants. To an excess of this power, in the vessels of the bark, we may also ascribe the peculiar growth of what are called weeping trees; for, by this power, the effects of gravitation will be, in a great degree, suspended; and the pendant branch will continue healthy and vigorous, by retaining its due circulation. The perpendicular branch will, however, still possess some advantages; for, in this, gravitation will act on the fluid descending from the leaves; and these will of course absorb from the atmosphere with increased activity. A greater quantity of matter will therefore enter, within any given portion of time, into vessels of the same capacity; and this increased quantity may frequently exceed

that which the vessels of the bark are immediately prepared to carry away. Much new wood will in consequence be generated, and increased vigour given; and, the same causes operating through successive seasons, will give the ascendancy we generally observe in the perpendicular branch.

In the preceding experiments, none of the layers, or cuttings, exceeded a few inches in length; and, to the summit of these the sap appeared to rise, through the inverted tubes of the wood, nearly as well as in those which retained their natural position. But some former experiments had induced me to suspect, that this would not be the case in longer cuttings; I therefore planted, in the autumn of 1802, twelve cuttings of the willow, (*Salix caprea*,) inverting one half of them. The whole readily emitted roots, and grew with luxuriance; but their modes of growth were extremely different. In the cuttings which stood in their natural position, vegetation proceeded with most vigour at the points most elevated; but, in the inverted cuttings, it grew more and more languid as it became distant from the ground, and nearly ceased, towards the conclusion of the summer, at the height of four feet. The new wood also, which was generated by these inverted cuttings, accumulated above the bases of the annual shoots, as in the preceding instances.

These facts appear to prove, that the vessels of plants are not equally well calculated to carry their contents in opposite directions; and, I think, afford some grounds to suspect that the vessels of the bark, like those which constitute the venous system of animals, (to which they are in many respects analogous,) may be provided with valves, whose extreme minuteness has concealed them from observation.

The experiments, and still more, the Plates, of HALE, have

induced naturalists to draw conclusions in direct opposition to the preceding. But the Plates of that great naturalist are not always taken correctly from nature;* and Plates, under such circumstances, however fair and candid the intentions of an author may be, will too often be found somewhat better calculated to support his own hypothesis, than to elucidate the facts he intends to state.

The preceding peculiarities in the growth of inverted cuttings, appear to have escaped the observation of DU HAMEL; and, as very few instances of error, or want of accurate observation, will ever be found in the works of that excellent naturalist, I must request permission to send you some of the subjects of my experiments, as vouchers for my own accuracy.

Of the inverted cuttings employed by DU HAMEL, a small portion only appears to have remained above the ground; and, under such circumstances, the different forms of those growing in their natural, or inverted, position would be scarcely observable. It appears also, from his experiments, that such inverted cuttings, in subsequent years, grow with as much vigour as others that are not inverted; whence we must conclude, that the organization of the internal bark becomes again inverted, and adapted to the position of the branch. The growth of some inverted plants of the gooseberry-tree, which I obtained, many years ago, from layers, gave me reason to draw a different conclusion; for these always continued weak and dwarfish. I do not, however, entertain the slightest degree of doubt, but that the assertion of DU HAMEL is perfectly correct.

I intended to have added some observations on the repro-

* The eleventh Plate (Vegetable statics) is that to which, in this place, I particularly allude.

duction of buds and roots of trees; but these would necessarily extend the present Paper to an immoderate length; I shall therefore reserve them for a future communication, and conclude with an account of an experiment which more properly belongs to the Paper I had the honour to address to you last year, but which had not then succeeded.

I have stated, in that Paper, that the leaf-stalk, the fruit-stalk, and the tendril, of the vine, had been successfully substituted, in many instances, for each other; but that I had failed in my efforts to engraft a bunch of grapes, by approach, on the leaf-stalk; owing, I conceived, to the operation having been improperly performed. In those experiments, I cut the leaf-stalk into the form of a wedge, and made an incision in the fruit-stalk, adapted to receive it; but, under such circumstances, the leaf-stalk (as I had proved by many experiments) has no power to generate new matter; and the wounds of the fruit-stalk heal so slowly, that I readily anticipated the ill success of the operation. In the last spring, I pared off similar portions of the leaf-stalk and fruit-stalk; and, bringing the wounded parts into contact, I secured them closely together, by means of a bandage, letting the leaf remain. Under these circumstances, an union took place; and the fruit-stalk being then taken off below the point of junction, and the leaf-stalk above it, the grapes drew their whole nutriment through the remaining part of the leaf-stalk. They did not, however, acquire their full size; and the seeds were small, and, I think, incapable of vegetating; but this I attribute to the want of nutriment in quantity rather than in quality; for the union of the vessels of the leaf-stalk with those of the fruit-stalk was very imperfect. The grapes, which

were the purple Frontigniac, possessed their musky flavour, in the same degree with others growing on the same plant.

There is another experiment in my last Paper, which I will also notice here, because it appears to lead to some important conclusions, and had been tried only in a single instance. I have there stated, that the stem of a young tree became elliptical, by being confined to move only in the segment of a large circle. This experiment was successfully repeated, during the last year, on other trees; but I have nothing to add to the description which I have already given.

I am, &c.

T. A. KNIGHT.